of three dimensions. In chapter iii. the author settles down to the composition, resolution, and equilibrium of a general system of coplanar forces, and gives a very good exposition of the subject; but in this chapter, of nearly fifty pages, no use of either the calculus or three-dimensional geometry is made, except in three pages devoted to the common catenary. Now the understanding of this very important and extensive section of dynamics is well within the power of any student even if he is quite ignorant of these branches of pure mathematics, so that it seems a pity that he should be kept back in his dynamical studies until he has passed through "an adequate course in the differential and integral calculus."

Passing over chapters devoted to the determination of centres of gravity and the composition of forces which are not coplanar, we come to chapter vii., which treats of the principle of work. This chapter is somewhat meagre, consisting mainly of what is known as "book-work," and not containing sufficient illustration of the applications of the principle to concrete cases. Until the student comes to chapter viii. he will experience no difficulty in the author's treatment of the subject; but when he reaches this chapter, on "motion produced by constant force," he will find a good deal about the nature of "inertia regarded as a force" which will be very perplexing. His main difficulty will be to decide whether the author means the "force of inertia" to be one exerted *by* a body or *upon* it by some agent or medium. Thus, at the beginning of art. 288 it would appear to be a force exerted *by* the body:—

"The property of matter through which *it resists* any change of motion, in accordance with the First Law of Motion, is called Inertia."

But a few lines farther on we have the sentence:—

"Now, just as the resistance of a fixed body in contact with that upon which the force acts, and preventing its motion, is regarded as a force equal and opposite to the force which would otherwise produce motion, so the resistance to motion in the body when free is regarded as a force equal and opposite to the active force which produces the motion."

Let us suppose a particle M acted upon by forces whose resultant is P and kept from moving by the resistance, N, of a fixed surface B; then the force N is exactly equal and opposite to the force P. Again, imagine the body M acted upon by the same force P and unresisted by any fixed surface; M will have an acceleration a, and the statement is that there is acting on M a force resisting the acceleration a—this force being clearly produced by something which in our thoughts replaces the above fixed surface B—that this force is equal and opposite to "the active force which produces the motion." So far, what this "active force" is is not clear; but the next sentence defines it:—

"Thus the force of inertia acts upon a particle of mass m only when there is an acceleration a, and its value is ma, while its direction is opposite to that of the acceleration."

Now observe that if the particle had no acceleration, this force would be zero, while in the first part of the analogy (where also $a = 0$) the supposed analogous force, N (the resistance of the surface B) is not zero.

However, from this and from subsequent statements it is clear that, in the author's view, a force of inertia really acts on a particle m which has an acceleration a, and that this force is scalarly and vectorly equal to $-ma$; that is to say, it is D'Alembert's fictitious "reversed effective moving force." But this is not in accordance with the statement at the top of p. 288:—

"And the inertia which acts upwards is, at that point, simply the resistance of the body to being moved away from the tangent at O."

It is certainly strange that a force acting *on* a body should be the resistance of the body to being moved. The author, however, clearly defines his conception, which he calls that of "kinetic equilibrium," at the top of p. 244:—

"For example, suppose a man whose weight is W to be standing on the floor of an elevator which begins to descend with the known acceleration a. The forces acting on the man are his weight, $W=mg$, acting downward, his inertia, ma, acting upward because the acceleration is downward, and the resistance R of the floor of the elevator acting upward. Since the forces are all vertical, there is but one condition of equilibrium, namely, $W=R+ma$."

The objection which a student will raise to this is that if the man is really acted upon by the upward force ma, the man is really at rest and not in motion at all.

D'Alembert never attributed anything but a fictitious existence to his "reversed effective forces," and he was right and consistent all through. The real objection to his principle is that it teaches us to be dissatisfied with the actuality (viz. motion), and to seek refuge in a fiction (viz. rest). The teaching of Newton's second axiom is quite different: it accepts motion as a fact and deals with it.

The remainder of the book gives somewhat short and easily readable discussions of central orbits, motion (especially uniplanar) of rigid bodies, moments of inertia, and impulses.

OUR BOOK SHELF.

The Earliest Inhabitants of Abydos; a Craniological Study. By D. Randall-Maciver. Plates viii + tables 16. (Oxford: Clarendon Press, 1901.) Price 10*s.* 6*d.* net.

IN the present work Mr. Randall-Maciver presents to the public the craniological material which he obtained in Upper Egypt in the winter of 1899-1900, and the results which he has deduced from it. In a series of eight plates he gives us photographs of a large number of skulls which he obtained from two cemeteries at Abydos, which, he says, belonged to the earliest and the latest stages of the pre-dynastic period, and to these he adds some sixteen tables of minute craniological measurements. The first cemetery contained only pottery of the earliest forms, black-topped, polished red, and white ornamented red, and the second degraded wavy-handled vases and other pottery of well-defined classes. The remarks which Mr. Randall-Maciver makes in his short preface may be regarded as a continuation of those expressed in his "Libyan Notes," and we observe that he still holds the view that the theory of the Libyan origin of the pre-dynastic or proto-dynastic Egyptians is "based on wholly inadequate evidence." The pre-dynastic Egyptians were, he thinks, a mixed race, but as a whole that race was not Berber; on the other hand, he does not deny the existence of an original Berber substratum,

though he believes that its existence requires to be proved. A question of the kind must be decided by "expert anthropologists," for "archæology has its own place, and should recognise its own limitations; it can prove connections of culture, but not identities of race." We can only hope that the archæologists who hold different views from those of Mr. Randall-Maciver will take these observations to heart and turn from the error of their ways. It is, we must confess, a little disconcerting to find such a strictly scientific authority as Mr. Randall-Maciver reduced to suggesting that "it is well worth considering whether the pre-dynastic race of Egypt is not in the main a blending in various proportions of Semite and Negro." It is much to be hoped that his promised work on the whole subject will clear up some of its difficulties, but it seems doubtful, judging by the work of Mr. Randall-Maciver and Prof. Sergi, whether the archæologist will obtain much useful help from the craniologist.

The New Basis of Geography. A Manual for the Preparation of the Teacher. By Jacques W. Redway. Pp. xvi+226. (New York: The Macmillan Company. London: Macmillan and Co., Ltd., 1901.) Price 4*s.* 6*d.* net.

NOT that the basis is really "new," for the author, who is perhaps the most successful writer of geographical school-books in the English tongue, knows a great deal better. In his preface he says in effect that the novelty of his basis is only apparent to the ignorance of the average teachers, and the newer they find it the more shame to them. "This book," he explains, "is intended to set forth in an elementary manner the relations between human activities and geographic environment." It does so very well. The style is facile and free, permeated by an air of genial familiarity with the subject, and with the class of reader appealed to. There is a tendency to semi-epigrammatic sentences, shattered fragments of which will be recognisable in the breccia of the pupil-teacher's examination papers for a generation to come:—"War has its horrors, but it is less horrible than ignorance." "Accuracy is the one virtue that cannot possibly belong to a flat map." "It is not necessary to worry about the plane of the ecliptic."

The last proposition will probably be popular, if one may say so without disrespect to other "imaginary lines."

Mr. Redway has produced a thoroughly practical, well-informed and thoughtful book; one which can not only be read with pleasure by the teacher in the study, but practised with profit in the school. True, it does not accord with any of the "codes" in this country, but the principles it lays down will be found perfectly amenable to any pattern of red-tape harness. Stress is laid on the superiority of the method of teaching by letting the pupil discover his own facts—"The reading method might fit a young man to be a private secretary; the discovery method fits him to be the employer of private secretaries."

References to books are given as well as hints on method, and Mr. Redway is generous in commending the works of other writers. We feel sure that his strongly practical exposition of the nature and value of geographical principles will do more to promote sound geographical education than any amount of learned advocacy by theorists can ever accomplish.

Expertises et Arbitrages. By F. Rigaud. Pp. 177. (Paris: Gauthier-Villars. Masson and Co., 1901.)

IN this volume, which belongs to the Encyclopédie scientifique des Aide-Mémoire series, the author gives a *précis* of standard legal works on reports and arbitrations, and summarises the principles and laws which should be considered by arbitrators and experts more familiar with technical knowledge than law. From this practical point of view the book may prove of service.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Note on a Point of Chemical Nomenclature.

SENIER AND GOODWIN have, in a recent paper (*Journ.* Chem. Soc. vol. lxxix. p. 258), made use of the term "alphyl" as a generic name for the aromatic radicles. The continued use of this term with the meaning attached to it by the above authors is one which may lead to some confusion. It is undoubtedly of advantage to be able to distinguish by special names fatty from aromatic radicles. With this end in view Bamberger proposed some time ago (*Berichte*, xxvii. 2583) "*alphyl*" as a general term for aromatic radicles, such as phenyl, tolyl, &c. He derived this word from "alkyl-phenyl," and as it supplied a want it was speedily adopted by investigators and appeared in scientific papers and text-books. Vorländer in 1899 (*Jour. für praktische Chemie*, lix. 247) drew attention to the possibility of error centred in the new name. As he remarked, every student of chemistry on hearing the word "alphyl" for the first time would think, not of an aromatic compound, but of one belonging to the fatty or aliphatic division. He then pointed out that *alphyl* was, on the contrary, a thoroughly suitable name for a monovalent hydrocarbon radicle of the fatty series, and that an aromatic radicle might be designated by "*arryl.*"

For monovalent fatty radicles we have the name "*alkyl*" suggested years ago by J. Wislicenus and derived from "alcohol." There is no reason for superseding that term, but its meaning might with advantage be enlarged. Vorländer's proposal was that *all* monovalent hydrocarbon radicles, whether fatty or aromatic, should be called "*alkyl*" groups, this term being in opposition to "*acyl*" used by Liebermann (*Berichte*, xxi. 3372) for acid-radicles. We may then subdivide the alkyl group into fatty and aromatic divisions, giving each a special name.

The following scheme sets forth the proposed nomenclature:—

I. *Alkyl.* All monovalent hydrocarbon radicles.

(*a*) *Alphyl.* Aliphatic radicles (CH_3, C_2H_5, &c.)

(*b*) *Arryl.* Aromatic radicles (C_6H_5, &c.)

(*c*) *Alpharryl.* Aromatic radicles possessed of a fatty character (benzyl, &c.)

II. *Acyl.* Acid radicles in general ($CH_3.CO$, $C_6H_5CH_2CO$).

Bamberger, the proposer of the term "alphyl" for aromatic radicles, acknowledged the ambiguity and adopted Vorlander's proposal (*Lieb. Ann.* cccv. 289). One modification he suggested, and this was the change from "arryl" to "aryl." Since that time he has used in all his work the term "aryl" where he previously used "alphyl." This is a custom which now generally obtains in Germany, and "alphyl" in its original sense has almost altogether disappeared from papers and text-books. Should now the term "alphyl" be used in England for an aromatic radicle, it will creep again into such reference periodicals as the *Centralblatt* and there occur side by side with "alphyl" and "aryl" in their later meanings, as has already been the case, and this will lead to manifest lack of clearness and confusion. Some English chemists use "aryl" for an aromatic radicle (*cf.* Sudborough, on acetylation of arylamines, *Proc.* Chem. Soc. xvii. p. 45). It would therefore be of great advantage to agree on a uniform use of these different terms.

A. T. DE M.

Folklore about Stonehenge.

I REMEMBER, when I was a child, between seventy and eighty years ago, being told that the stones could be successfully counted only by laying a loaf of bread beside each. To mark each stone by something to prevent one being missed or counted twice over seems natural; but why a loaf of bread? Is this an idea surviving from the "*cultus lapidum*" referred to in your review of "Carnac and Stonehenge" in NATURE of September 12? I think it probable that I had this from a nursery-maid who came from Mere in Wiltshire, and who had a taste for the marvellous.

O. FISHER.

Harlton, Cambridge, October 19.